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Commissioner for Patents

The Appendix that you have requested is enclosed.

Nikita Wells
Primary Examiner
Art Unit: 2881

02/15/07

Response to Request for Continuing Examination SN 09/513,768
Bernard E. Souw – Patent Examiner AU 2881
January 15, 2004

The following is a response to Applicant's arguments against the key analysis provided in the Appendix attached to the May 7 Advisory Action.

(a) Regarding Applicant's Remarks in the RCE

(1) Regarding Applicant's remark in RCE page 10 of 68, reciting "*new standards imposed by the PTO on evaluating the credibility of Applicant's technical papers*", it is to be emphasized that there is **no** "*new standard*" applied in this patent examination: Applicant's invention is deemed *incredible under 35 U.S.C. §101* for violating known laws of nature, and therefore, the burden is on Applicant's side to establish credibility, either by hard evidence, i.e., a working prototype device, or by credible and reliable supports, i.e., peer-reviewed publications in refereed scientific journals. This 35 U.S.C. §101 is not at all an "*arbitrary*" standard, as alleged by Applicant on pp.7-8 of the RCE, but a fundamental law that sets a standard for patent examination (see MPEP 706.03(a)).

Applicant's further remarks on pg.10, that "*it is only fair that the reference materials cited by Examiner Souw in the Appendix attached to the May 7 Advisory Action be subjected to the same standards*", then setting forth on pp. 11-12 that technical papers cited from the journal *Physica* are "*not more scientifically qualified with appropriate review process than the journals that published Applicant's papers*" is again unpersuasive. The journal *Physica* is a

refereed journal, published by the reputable and widely known European publisher, Elsevier, which also publishes about 100 other scientific journals.

Applicant's further remark that *"many" of the reference materials cited by Examiner Souw in his Appendix to the May 7 2003 Advisory Action have not been published in accredited journals and/or "most certainly" have not been peer-reviewed*, is untrue. Only 2 out of 13 references are not peer reviewed, i.e., refs. (1) and (12), and their citation is justified as follows:

Ref.(1), regarding the He-II 304Å line, is just one example out of an overwhelmingly large number of other (refereed) publications. The He-II 304Å line is a very well known spectral line routinely measured in many observations, so that one example is here sufficient. Furthermore, the line is also listed in many spectral tables.

Ref. (12) may be simply discarded, since it has very little relevance. This means, the last sentence in lines 7-8 on page 12 of the May 7 Appendix may be omitted without any consequence.

Regarding Dr. P. Zimmerman's allegedly non-peer-reviewed paper, Applicant's own explanation on page 10 sufficiently provides his acknowledgement that the paper is well reviewed, not just by a few referees, but by the scientific community. Other non-journal references cited in the May 7 Appendix are official publications of reputable universities and institutes, such that dismissing them would automatically dismiss the issuing institutions.

(2) Contrary to Applicant's allegation on pg.13, 1st full paragraph, lines 2-4, the PTO's view is not at all that the existence of lower-energy hydrogen were impossible, but instead, that (a) Applicant's invention is not supported by any experimental fact or evidence, and (b) the underlying theory (i.e., GUT/CQM) fails to support the invention, because it contains too many flaws. A few of such flaws have been already exposed in the May 7 Appendix, *none* of which have been persuasively argued or refuted by Applicant in the RCE, as will be demonstrated in the next section(s).

(3) Applicant's allegation against Examiner Souw's technical publications cited in the Appendix as being allegedly "*a subject of Applicant's criticism*" is misaddressed, since those papers have nothing to do with Applicant's claim(s). They merely provide support to the Examiner's viewpoint through evidence of sufficient proficiency in Quantum Mechanics. They also expose the inadequacy or factual incapacity of Applicant's GUT/CQM to derive comparable formulation for transition probabilities and line intensities, complete with verifiable selection rules, of which the conventional QM is proven capable, as demonstrated by the Examiner's own experimental and theoretical works cited in the May 7 Appendix. A classic example is here the famous work by E.H. Condon and G.H. Shortley, "The Theory of Atomic Spectra", Cambridge 1967, which serves as a standard to most advanced spectroscopists until today. One cannot dismiss an established and evidently successful theory without offering a better alternative. Therefore, Applicant's GUT/CQM remains incredible; or speculative at best.

Since the cited Examiner's papers cannot possibly have been criticized by Applicant, citing his own publication(s) does not make the Examiner's view "*biased*", as alleged by Applicant. In the contrary, such technical papers provide a solid evidence that the Examiner is in possession of sufficient background for evaluating Applicant's claimed invention. In this regard, the Examiner can add a further evidence of strong background in microwave plasmas generated in a resonance cavity similar to those used by Applicant, not only in theory, but also hands-on in its design, construction and routine operation, as well as in its spectroscopy, both low and high resolutions [1].

Applicant's insistence that the present application be examined only by those Examiner(s) who have ***no*** pre-knowledge of QM is unpersuasive, since such an Examiner would not be able to give any evaluation --let alone, judgment-- on the patentability or credibility of Applicant's invention. In a normal legal process, a jury member's judgment may be biased under a pre-knowledge of the *case*, but still, he/she must be knowledgeable in the *subject matter*. In a patent examination process a pre-knowledge of the relevant subject matter lends the Examiner a better capability to make proper judgment about the credibility or patentability of the claimed invention.

(4) Regarding Applicant's allegation on page 12, 5th full paragraph, that "*Examiner Souw prefers engaging in theoretical debate*", it is to be noted that a patent examination is *not a debate*, but an *evaluation* of an invention with regard to its novelty and patentability, or --in a negative case-- its incredibility.

Furthermore, the Examiner's refutation presented in the May 7 Appendix is not just theoretical, but also experimental, among others, regarding a He-II line that has been misidentified by Applicant as a hydrino line (Appendix page 3, 1st paragraph), also regarding a contamination peak in Applicant's XPS spectra that has been misidentified by Applicant as being due to a *hydrinoide* compound. (this terminology –meaning a hydrino-based "*hydride*"--, is here preferred rather than Applicant's terminology "*novel hydrogen compound*", for which there is no evidence for its existence, neither experimental nor theoretical).

It is noted, none of the experimental refutations presented by the Examiner in the May 7 Appendix has been persuasively and successfully argued by Applicant. This will be addressed in more details in the following Examiner's Response to Applicant's arguments against the Appendix attached to the May 7 Advisory Action.

(b) Regarding Applicant's arguments against the 05/07 Appendix

(1) Applicant's argument on page 24, 1st full paragraph, with regard to impurity peaks in XPS spectra, in which Applicant is questioning *the Examiner's "credibility"* for allegedly proposing *"no alternative assignment"*, is unpersuasive. The Examiner has given a *definitive* (not just alternative) assignment to Applicant's hydrinoide peak, i.e., as a *contamination* or *impurity* peak, complete with its experimental evidence, i.e., the fact that it *disappears after the sample surface has been cleaned*, as recited in May 7 Appendix, page 4, lines 9-12.

Applicant's argument (pg.24, 1st full paragraph, lines 1-5) that a TOF-SIMS

pre-analysis conducted on the sample "*does not show any impurities*" is unpersuasive, since it does not prove that Applicant's sample is uncontaminated, but merely indicates that Applicant's pre-analysis itself is *deficient* (Applicant's XPS spectrum, in agreement with ref.(2) of 05/07 Appendix, does show such contamination!).

It is well-known in the art that TOF-SIMS is more adapted to mass-profiling, owing to its high mass resolution capability, whereas XPS is more suitable for surface contamination analysis, due to its high surface sensitivity [2]. A typical TOF-SIMS surface sensitivity or resolution is 0.15 μm, i.e., 50 times worse than XPS (typically 0.003 μm [2]/pg.1). Apparently, Applicant's sample was contaminated due to inadequate precautions during its preparation, as advised in ref. [3]/pg.2 (base pressure must be below $2 \cdot 10^{-10}$ mbar). Applicant's confusion with his own XPS results has been further enhanced by the well-known variability of the XPS peak position with regard to valence/oxidation state of the atom and its chemical environment [4]/pg.1, as well as due to surface charging [3]/pg.4, altogether finally led to Applicant's peak misidentification.

(2) Applicant's argument on page 24, 1st full paragraph, lines 6-9, is unpersuasive because it has no substance, seemingly an attempt to defend and justify Applicant's obvious misidentification of the 304Å He-II line that has been already pointed out in the May 7 Appendix, page 3, 1st paragraph. Applicant's "new evidence" in the form of recent (2003) publications on EUV spectra from a microwave discharge of He/H mixture does not change the fact that Applicant has factually misidentified the 304Å line as being due to a hydrino transition.

Regarding Applicant's claim of other EUV lines in a 2003 paper (RCE ref.(50)), the Examiner does not see any compelling reason why they must be due to hydrino transitions, since there are many other possible origins that are conventional, as also addressed in the main part of this Office Action. In this regard, Applicant's explanation as to how a catalytic *reaction* (?) with, or *resonance* (?) transfer to helium ions (ionization energy 54.4 eV) could possibly induce a *nonradiative* transition of *atomic* hydrogen to n=1/2 ($\Delta E < 54.4$ eV !), and then followed by *radiative* transition further to n=1/3 or lower, is not persuasive (starting with which n? Hopefully not $n=\infty$, since that "*resonance*" is *not a transition*, but electron capture!).

In short, to attribute those new lines to hydrino origin is speculation. Applicant is here trying to speculatively interpret a vague experimental observation to justify his own hypothesis of hydrino.

(3) Regarding the Balmer α line broadening recited on page 24, last paragraph, and page 25, 1st – 6th paragraphs, Applicant's arguments are unpersuasive, because:

(i) the Black Light Process Theory is not a valid argument or support, for being based on a flawed theory not accepted by the scientific world, i.e., the GUT/CQM, also for lack of experimental evidence. Furthermore, to prove something using the same thing that is to be proven, is a classic *Circular Reasoning*.

(ii) Regarding the alleged hydrino origin of Applicant's hydrogen line broadening, it is too well-known that hydrogen transitions are easily perturbed by the plasma and microwave fields, since the atomic hydrogen has only one electron that is not protected by screening effects, especially those having large l quantum numbers. Especially in microwave plasmas, anomalous broadening of hydrogen lines (also of helium lines) has been subjected to experimental and theoretical studies for *decades* [5].

In this regard, Applicant's repeated referrals to *his own* measurements of such anomalous broadening as an "*overwhelming evidence*" for the existence of hydrino, is not persuasive in at least two aspects.

Firstly, Applicant's reports reciting catalytic reaction involving hydrino as the origin for the line broadening observed, are mostly non-refereed publications, as already identified in the previous Office Actions. This also applies to the new references presented in the RCE (see main Office Action). Those published in peer-reviewed journals do not mention "hydrino", or "hydrogen states with fractional energy levels", or anything the like, and therefore, they cannot be considered as "support". In a very few cases, ambiguous term(s) are used by Applicant, so they can neither be considered support for Applicant's hydrino, since what a person of ordinary skill in the art understands under, e.g., "*novel catalytic reaction involving atomic hydrogen*", or "*new kind of plasma*" may be anything else but not related to Applicant's "*hydrino*" as hypothesized in the GUT/CQM. For example, Applicant's most recent reference [6] (RCE ref.(49)) does not recite the word "*hydrino*", or "*hydrogen of fractional energy level*", or the

like, nor does it refer to GUT/CQM at all. Consequently, the phenomenon described in such papers must be considered as being completely unrelated to Applicant's invention.

Secondly, and *most importantly*, anomalous hydrogen line broadening is not at all an evidence for the existence of hydrino, because it is well known in the art that such a broadening may be caused by many other conventional mechanisms, such as microwave plasma effects, the latter having not been considered by Applicant. Instead, such an effect has been sofar ignored or dismissed by Applicant without any valid reason. The measured excessive line-width shown in Applicant's Fig.6 of ref.[6], i.e., 0.27 nm, is about the same magnitude as what is measured by other authors, e.g., ref.[5] cited in the May 7 Appendix, here reproduced in Fig.1 below.

As shown in Fig.[1], the anomalous line width of 0.16 nm, measured in a microwave discharge similar to Applicant's under the same gas mixture and pressure range, is about 10 times the Doppler width, and has been attributed to microwave plasma effects. Applicant's previous criticism on this reference [5] is unjustified. The paper was a conference publication posted by a research group from the University of Essen, Germany. It is not a lone-standing unrefereed paper as alleged by Applicant, but instead, supported by a large number of experimental and theoretical works, as listed at the end of the paper. It would have been extremely doubtful to one of ordinary skill in the art if any cause other than the RF/microwave field itself should be held responsible for an effect that is only and exclusively observed in RF/microwave plasmas.

Fig.1 (from ref.[5])

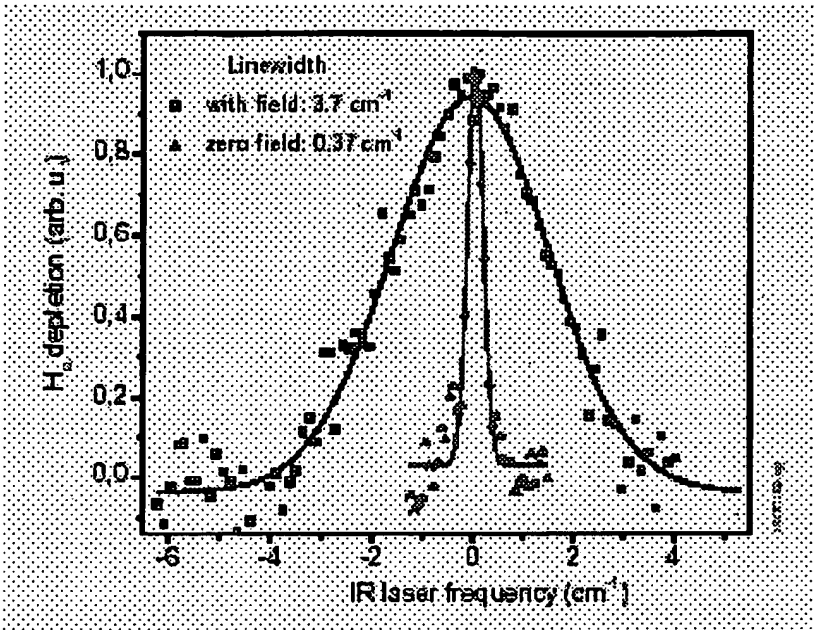
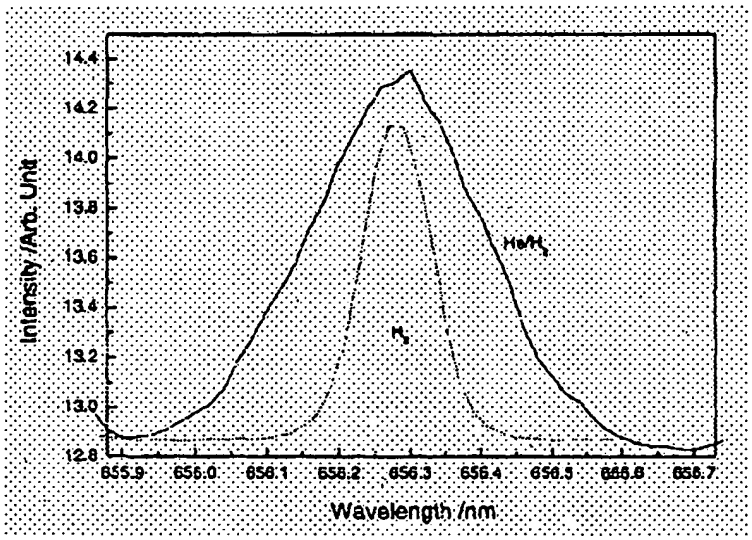


Fig.1: Line broadening in the microwave field in comparison to the field-free line shape.

Fig.2 (from Applicant's recent publication [6] Fig.6)



Thus, Applicant's "*excessive line broadening*" is not at all an evidence for Applicant's hypothetical "hydrino", but rather, a conventional effect that has been already observed by other authors, as evidenced in Fig.1. Such a high frequency Stark effect has *never* been considered in any of Applicant's numerous reports on "anomalous" or "excessive" line broadening. Instead, Applicant always assumes that Doppler effect were the main cause, as clearly stated on pp. 6-7 of Applicant's Appendix attached to the RCE. The assumption of Doppler effect as a main mechanism for Applicant's line broadening has been known as incorrect for RF and microwave plasmas, as discussed above. Applicant's mis-assumption of this line-broadening mechanism automatically disqualifies all Applicant's arguments based on anomalous or excessive line broadening in microwave plasmas.

Applicant's statement on page 6, that "The Examiner does not present any alternative explanation for the cited data", is doubly unpersuasive. Firstly, alternative explanation was readily provided by the result shown in Fig.1, which has been explicitly cited in the previous May 7 Appendix [5].

Secondly, there is no obligation for the Examiner to provide "*alternative explanation*" as demanded by Applicant, since a patent examination is neither a scientific debate nor a scientific research, such that Applicant's demand is improper. Similar to the case of the misidentified XPS contamination line, for the purpose of patent examination only the factual evidence is relevant, i.e., that the broadening is *not* due to Applicant's hypothetical hydrino. Any attempt to rely on such hypothesis (notwithstanding its theoretical flaws), where there is already

another theory that is scientifically sound and experimentally verified, is essentially unjustified, and hence, must be deemed incredible, or speculative at best. After all, it is Applicant's invention that is deemed incredible under §101, so the burden is on Applicant's side to prove his credibility.

(iii) Inversion of line intensities, either of the Balmer series or other series, is well known in laser and plasma physics since many decades. As already recited in the previous Office Action, one of ordinary skill in the art should better look for other conventional mechanisms as a cause for inversion of line intensities and enhanced EUV emission, instead of making a premature claim of a hypothetical hydrino. Furthermore, the references named by Applicant do not specifically recite the unambiguous word "hydrino", or "hydrogen states with fractional energy levels", or anything the like, so they cannot be considered as support for Applicant's hydrino hypothesis.

(iv) Novel VUV vibration spectra, too, may be caused by conventional effects, such as molecular gas dynamics, i.e., an exchange of translational and vibrational energies, which is the principle of gas dynamic lasers, and has nothing to do with hydrinos.

(v) Waterbath calorimetry is an outdated method that has proven unreliable for demonstrating the efficiency of energy generation, as evidenced by the infamous failure of the cold fusion experiment, which also relied on such calorimetry, but could not be reproduced by anyone else.

(4) Applicant's misinterpretation of the Examiner's statement of a *stationary state* as being of a ***static*** *electron*, i.e., an *unmoving* electron (Applicant's Response, pg.27, 2nd paragraph, lines 1-2 and 5-6) is a grave *misrepresentation*. No person skilled in the art would ever misunderstand such a simple, conventional terminology like "*stationary state*".

(5) Given two distributions of positional probabilities, $|\psi_1|^2$ and $|\psi_2|^2$, it is quite obvious to one of ordinary skill in the art that the trajectory of an electron in an upper state ψ_2 may easily, and smoothly, "slip" or transit into the trajectory of a lower state ψ_1, and that the probability for such a transition is proportional to the amount of a spatial overlapping of the two (complex) probability distributions, $\psi_1^* \psi_2$. Given the further fact that –due to energy conservation-- the transition is a direct result of, and hence, must be simultaneously accompanied by electromagnetic radiation, a quantitative formulation for the transition probability must involve an electric dipole (or multipole). Thus, the dipole transition probability is expressed by the dipole component of the overlapping, i.e., $\int (\psi_1^* \cdot er \cdot \psi_2)\, d^3r$. This formulation is solidly supported by the fact that no transition is observed between two spherically symmetric states (l =0), since the transition dipole is zero, although the two distributions do overlap. The correctness of conventional QM is ultimately proven by the fact that highly complex quantum theoretical calculations of transition probabilities and line intensities has been experimentally verified to great accuracies, as demonstrated in the May 7 Appendix. This is to be compared with the factual incapacity of GUT/QCM for doing the same, as discussed previously.

Thus, in contradiction to Applicant's allegation, it has been demonstrated that the rationale behind the atomic transition, including its quantitative formulation, is completely logical and can be understood by one of ordinary skill in the art, as world-widely confirmed by all scholars in quantum physics.

(6) Applicant's use of the Haus's formula is based on a misunderstanding of the electromagnetic theory, i.e., (a) confusing electromagnetic wave fields with charge density by putting the charge density ρ in place of electric field E in the Helmholtz equation containing the Laplace operator (GUT/QCM pg. 48/Eq.1.1 and Applicant's Response to the May 7 Appendix, pg.35/Eq.2), and (b) confusing current density with charge density, i.e., by putting a charge density ρ in place of current density $\boldsymbol{j}$ in Haus's Eq.6. In an attempt to justify his position, not only Applicant has failed to correct the relevant mistakes, but he further introduced new inconsistencies in regards of the fundamental concept of physics, i.e., by equating the current density $\boldsymbol{j}$ with the product of charge density ρ and angular velocity ω, under the wrong assumption that "*in the case of harmonic motion the current density function is given by the time derivative of the charge density function*", such that "*the current density function is given by the product of the constant angular velocity ω and the charge density function ρ*" (pg.40/lines 2-7 from bottom), i.e., Applicant meant $\boldsymbol{j} = d\rho/dt = \rho_o \cdot d(\exp -i\omega t)/dt = \rho \cdot \omega$. However, a quick & simple dimensional analysis shows there is a unit of length missing in Applicant's formulation. The correct formula is $\boldsymbol{j} = e \cdot \rho \cdot v = e \cdot \rho \cdot \omega \cdot r$, which results in $[C]\,[m^{-3}]\,[sec^{-1}]\,[m] = C/m^2/sec$, bearing the proper unit of current density. Note further that <u>neither ω nor r is constant</u>, since the <u>constant of motion</u> is here the

angular momentum ~ ωr^2. Thus, Applicant's formulation of the Haus's condition remains incorrect as stated in the 05/07 Appendix.

(7) As a last test, to evaluate one more of Applicant's responses to the Examiner's 05/07 Appendix, i.e., on Applicant's misunderstanding of the Special Relativity Theory by incorrectly applying the Lorentz contraction formula on an orbiting electron, where it is well known in the art that Special Theory is only valid for linearly moving systems without acceleration. In an attempt to justify his initial violation of physics law, Applicant sets even forth with violating another fundamental principle of special relativity by postulating a (radial) velocity equal to c (pg.41/lines 1-2 after Eq.1.44). Firstly, a velocity equal to c would make the electron mass infinite. Secondly, if the radial projection or component of the velocity is equal to c, then the magnitude of the electron's velocity must be even larger than c, which then means Applicant's electron is a "tachyon" that does not belong to our real world. Thirdly, Applicant is violating the QM by postulating an electron wave number k that is equal to ω/c, i.e., having no dispersion (pg.31/lines 2-4 after Eq.1.65 and pg.42/1st full paragraph, lines 2-3). Applicant's proof of Haus's non-radiative condition by substituting Eq.1.45 into Eq.1.40 with $k=s=\omega_n/c$ and $r_n=\lambda$, which results in $\text{sinc}(2sr_n) \rightarrow \text{sinc}(2\cdot 2\pi)=0$, is only possible under the (wrong) assumption of a dispersionless wave $k=\omega_n/c$, which is only valid for photons, but invalid for electron and material waves in general. Being a material wave, the electron *must have a dispersion*, with a group velocity V related to the frequency ω according to the dispersion formula $V=d\omega/dk$, where k is the wave number, defined as $k=2\pi/\lambda$, and λ is the *de Broglie* wavelength,

related to the particle momentum p as $\lambda=h/p$ with h=Planck's constant, thus resulting in $p=hk/2\pi=\hbar k$. We thus have the energy-momentum relation $E = p^2/2m + U = \hbar^2k^2/2m + U$, where U is the potential energy. In QM, the particle's energy is related to the particle-wave angular velocity ω as $E=\hbar\omega$. This results in a dispersion relation for material waves, $\omega = \omega(k) = (\hbar^2k^2/2m+U)/\hbar$, which also means that material waves is always dispersive, $k = [\sqrt{(2m(\hbar\omega-U)}]/\hbar$. In other words, k can never be a constant in terms of ω as postulated by Applicant in lines 2-3 of the 1st full paragraph on page 42, i.e., $k=\omega_n/c$ only valid for mass-less photons, but not for electrons. Even if Applicant's assumption of $r_n=\lambda_n$ were accepted (in fact it is wrong, because of said violation of special theory of relativity), substituting $s=k=[\sqrt{(2m(\hbar\omega-U)}]/\hbar$ into Applicant's Eq.1.40, we obtain a condition $K(s,\omega) \sim \sin(2s\cdot r_n) = \sin(2\lambda_n\sqrt{[(2m(\hbar\omega-U)}]/\hbar)$ <u>*which does not identically vanish*</u> as Applicant (wrongly) postulated on pg. 42/1st full paragraph, lines 1-2 (consequently, also in GUT/CQM). Consequently, relying on Applicant's own words, the hydrino state should be radiative, too.

Apart from the fateful mistake described above, Applicant's condition for vanishing $K(s,\omega)$ is mathematically and conceptually incorrect, for being only fulfilled for a perfectly circular electron orbit. For general circular motion, the argument of Applicant's ***sinc*** function, $2s\cdot r_n$, is not constant in time, because it is not a constant of motion, as discussed previously. Thus, Applicant's non-radiative condition applies only to a perfectly circular electron orbit (pure classical mechanics). General hydrino states must be radiative, in self-contradiction with Applicant's initial *postulate* (not a first principle).

We see, in this one small part of Haus's radiationless condition alone, it has been proven that Applicant's GUT/CQM contains numerous mathematical and conceptual *errors*, all these *in addition* to those already discussed in the previous 05-07 Appendix, which also have not been properly argued. Moreover, Applicant's arguments not only violate the QM, but also the special relativity theory, electrodynamics, classical mechanics, and the classical wave theory as well.

References:

[1] "Plasma density measurement in an imperfect microwave cavity", E.-K. Souw, J. Appl. Phys.61(5), 1987, 1761-1772.

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[6] "Comparison of Excessive Balmer α Line Broadening of Inductively and Capacitively Coupled RF, Microwave, and Glow-Discharge Hydrogen Plasmas with Certain Catalysts", Mills et al., IEEE Trans. Plasma Science 31(3), 2003, 338-355.